An Example of Spurious Correlation.

IF I am not mistaken, the first method of forecasting the summer season proposed by Mr. A. B. MacDowall in NATURE of September 16, 1909 (vol. lxxxi., p. 335), is based upon a spurious correlation. If we take a series of departures from normal of a meteorological element and tabulate the sums of consecutive groups of thirty, there will always be a relationship between these sums, although the original departures may be entirely independent, and hence the relationship between the sums cannot be utilised for forecasting an individual term of the original series. That such sums of independent departures are not independent may be seen in the following way. If we denote the original independent departures by d_1, d_2, . . ., and the sum of thirty quantities beginning with d_p by s_p, the correlation coefficient between such quantities as s_p and s_{p+1}, as given by statistical methods, will be the mean value of a long series of products $s_p\, s_{p+1}$ divided by the product of the square roots of the mean values of s^2_p and of s^2_{p+1}. Now as d_p, d_q are independent the mean value of the product $d_p\, d_q$ will be zero; and it is easily seen that the correlation coefficient in question is the mean value of $(d^2_{p+1} + d^2_{p+2} + \ldots + d^2_{p+29})$ divided by the product of the square roots of the mean values of $(d^2_p + \ldots + d^2_{p+29})$ and of $(d^2_{p+1} + \ldots + d^2_{p+30})$; if we denote the mean value of d^2_q by m^2, this becomes $29m^2/30m^2$, or 29/30. Thus the thirty-year sums of independent annual departures will tend to vary closely together, and the dots in a diagram like that of p. 335 would tend to lie on a straight line.

The relationship actually found by Mr. MacDowall between the sums does not appear, therefore, to afford a satisfactory basis for a forecast. GILBERT T. WALKER.

India Meteorological Department, Simla,
December 16, 1909.

On Fluorescence Absorption.

IT is desirable to direct attention to Prof. R. W. Wood's most important paper in the *Philosophical Magazine* for December, 1908, on a method of showing fluorescent absorption directly if it exists; but it seems certain that he has, at the end, drawn a conclusion from his experiments the very opposite, as I venture to think, to that to which they really lead. He compares the light apparently transmitted by a fluorescent body when fluorescence is, and is not, taking place, and finds that there is no difference in the resultant effect. This, I think, is as it should be; but the inference he draws is that there is no difference in the absorption. For my part I must admit that it only confirms my results published in the Philosophical Transactions, vol. cxci., A, 1898, that there is such an absorption; for if there were none such the light *apparently* transmitted would be less when the body is not fluorescing, owing to the fact that the fluorescent light would increase the *apparent* transmission, and a flickering should ensue; but Wood's experiment demonstrates that this is not so. The inference I should draw, then, is that during fluorescence there is an increased absorption of the light transmitted.

Prof. Wood appears to assume, moreover, that the resultant effect on the retina of two successive flashes is equal to the sum of the two acting simultaneously, which is not the case, since the successive flashes act merely as an intermittent single flash would do.

Nichols and Merritt, who have fully confirmed my results spectroscopically, have shown that the absorption effect diminishes as the intensity of the transmitted light increases, so that when the intensity of the transmitted light is large in comparison with that of the fluorescent light there is no effect at all, owing to the fact that this transmitted light itself is sufficiently intense to excite fluorescence, and there is therefore no change of state in the two cases.

If uranium glass is used for the absorption—it was with uranium glass that I observed the effect—the source of the transmitted light should also be uranium glass. I may add that the best results were obtained by using the light from the spark between cadmium electrodes for exciting fluorescence. With a suitable Leyden jar in the circuit, the illumination is sufficiently steady, and any errors in this respect can be detected by the null method I have described. J. BUTLER BURKE.

December 18, 1909.

Adsorption.

"THE above effects, however, become of consequence in those frequent cases in which a muddy liquid is only partially filtered through a dry filter in order that some analytical estimation may be made in a given volume of the filtrate. *The first drops of the filtrate must therefore be discarded.*" The above quotation is from Ostwald's "Foundations of Analytical Chemistry" (English translation), the italics being in the text. Ostwald makes this a purely theoretical deduction, but the practice of discarding first drops does not, I think, originate with him. Doubtless many analysts neglect the precaution, but many use it.

Some experimental work on adsorption which I am at present carrying on seems to point to the practice being quite uncalled for in at least the majority of cases. I am not yet ready to speak definitely, but it seems to be as unnecessary as it would be to reduce weighings to a vacuum standard in everyday analytical work. Ostwald's extreme positiveness, however, makes me wonder whether I have overlooked some serious fault in my experimental methods, and I should be much indebted to anyone who would point out to me any references in the literature which give an *experimental* justification of the practice. The absence of any library facilities in this place makes a systematic search of the literature impossible to me.

ALFRED TINGLE.

Imperial Chinese Pei Yang Mint, Tientsin,
December 8, 1909.

The Terminal Velocity of Fall of Small Spheres in Air.

AT the Winnipeg meeting of the British Association Prof. Zeleny and Mr. McKeehan read a paper on the terminal velocities which they had found when Lycopodium and other small spores fall through air. The measured terminal velocities were only about half those calculated by Stokes's formula. The fall was steady, no Brownian motion or rotation being visible. The authors of the paper have since succeeded (see NATURE, December 9, 1909, p. 158) in making minute spheres of wax and mercury which do obey the theoretical law, but add that the reason for the deviations in the former cases is not clear.

May not the reason for these deviations be the roughness of the spore? The drops, through surface tension, are smooth and practically perfect spheres, whereas a spore of Lycopodium is very rough relative to its size. (Using a microscope objective with large aperture, and oblique illumination, Lycopodium spores of about 14 μ radius were seen to be coated with hair-like projections.) The spore would, from its roughness, leave a tail of small eddies behind it. The increased energy of this turbulence represents the increased resistance which the spore experiences on account of its roughness, as compared with that experienced by the smooth drop considered in the theoretical law, much as the speed of a ship is lessened when its bottom is foul.

As a suggested experimental test, an increase in the pressure of the air will not affect the viscosity, but will alter the energy in this tail of small eddies. So also would a moderate decrease in the pressure, while yet it would probably not bring the relative size of the spore and of the gaseous molecular free path too close for the theory to be applicable. Should this be the case, however, it would be shown by the appearance of Brownian motion. EDITH A. STONEY.

Positions of Birds' Nests in Hedges.

LIEUT.-COLONEL TULL WALSH'S observations as to the positions of nests (NATURE, December 16) are interesting, as they tally with the aspect of arboreal cryptogams, as already noted by me. South-west winds depositing sulphurous and nitrous products to leeward of towns cause lichens and mosses to flourish best on the eastern side of trees and hedges; and, moreover, this is general, for winds bearing spores from the south-west continually play on the trunks and blow away spores as they settle. If it were not for a kind of capillary attraction or rotary motion drawing the spores round the trunk to leeward, or east or north-east, they would never germinate. So the eastern side is the most productive, though often the western

aspect may exhibit a greater abundance of species, though less well developed, from the continuous play of spores—and rain—upon the trunk.

Of north and south positions the same may be said, *i.e.* the south is sheltered from fierce, cold north winds, yet open to warm, rain-depositing winds. Again, once established, cryptogams flourish on the southern aspect best owing to its sunny character. North winds blow when spores are not so abundant, and the same applies to boisterous east winds, though these are short-lived.

Apart from wind dispersal of spores, vegetative reproduction tends to favour the same situations, south or east, for south-west winds bring moisture, and, when not laden with poisonous substances, are beneficial; but long-continued wind tends to drive plants to the east side, and absence of sun from north to south.

The causes inducing birds to nest preferably on the eastern and southern sides of hedges (and trees, to some extent) are much the same, *i.e.* protection from wind and the greater safety of a leeward position and amount of sunlight; but in their case, also, there is light dispersal. There is a shadow on the leeward side of hedges for a great part of the day after the early dawn, and this enables birds safely to go in and out without being observed.

The western side presents fewer convenient nesting sites, the branches of hedges being generally bent over from west to east, as seen best on the west coast, affording a better arbour on the east. The south and east face early dawn longest, and this is the favourite season of the birds. At any rate, their song is richest between 4 and 8 a.m. The north and west are open to bright sun but during the *colder* part of the day.

As to the actual distribution of nests, the same positions noticed by Lieut.-Colonel Tull Walsh are favoured by birds in Leicestershire, Shropshire, Surrey, amongst other counties, and seem to be more or less general. The need for studying cryptogamic distribution in relation to wind in connection with the extinction of plants led me to formulate the conclusions noted. It is interesting to observe that they are directly analogous to the position of birds' nests in hedges. Hence the parallel drawn.

A. R. HORWOOD.

Leicester Corporation Museum, December 22.

Studies in Polychæt Larvæ.

MAY I make use of your columns to correct an error in my "Studies in Polychæt Larvæ" in a recent number of the *Q.J.M.S.*? The specimen there described as a young Odontosyllis sp. I have since found to be in reality a fully grown Exogone. I have been unable to identify it with any known species, but as dorsal natatory setæ are quite well known in specimens of Exogone of this size, the conclusions drawn from this specimen are of no value.

With regard to the last section of the same paper, it has lately come to my notice that de Saint-Joseph has shown Claparède and Mecznicow's so-called Spionid larva, in which there are no provisional setæ, to be the larva of the aberrant worm Saccocirrus, and not of one of the Spionidæ. There is, then, no known exception to the rule that free-swimming Spionid larvæ bear provisional setæ.

F. H. GRAVELY.

5 Silver Street, Wellingborough, December 27, 1909.

Cross-fertilisation of Sweet-peas.

I HAVE recently seen two further reiterations of the statement that the sweet-pea is invariably self-fertilised, a statement which I think is often based on an opinion of Charles Darwin's. It may therefore be worth while placing on record an observation made by me in 1907, when examining daily and closely a large quantity of sweet-peas. While *Apis mellifica* failed entirely to open the flower, it was done perpetually by *Megachile willughbiella*, and there was not the least doubt about the cross-fertilisation being effectively brought about by this bee. The point has probably been noted before, but it is worth recording once more in view of the repetition of statements as to the self-fertilisation of sweet-peas.

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A Supposed New Mineral.

WHEN we wrote recently (NATURE, October 28, 1909) about a supposed new mineral from Co. Antrim, we were led to believe that the specimen we had received was from the basalt—our information, in fact, was that "a very big pocket of it" had been found in that rock, but the exact locality could not be ascertained at that time, the finder having left home.

We now hear that this gentleman cannot remember where he got the specimen. It certainly cannot be traced to the basalt, and as its composition is unlike that of any known mineral, it seems highly probable that the substance is an artificial product.

RICHARD J. MOSS.
HENRY J. SEYMOUR.

Laboratory, Royal Dublin Society,
December 23, 1909.

THE HEART OF ANTARCTICA.[1]

IMMEDIATELY after the arrival of the British Antarctic Expedition of 1907–9 in New Zealand the attempt was made in NATURE (April 1, 1909, vol. lxxx., p. 130) to estimate its scientific results from the information received by cable. The full details now supplied show that the estimate then made in no way exaggerated the greatness of its achievements. The full story of the expedition, told in these most interesting and beautifully illustrated volumes, shows that its great success was due to careful and scientific foresight in equipment, to the determined and uttermost use of the equipment and staff, and to daring in the field, carried sometimes to the verge of recklessness, but saved from accidents by sound judgment and cool courage.

The main purpose of the expedition was to reach the South Pole, and as that object required an advance into the heart of Antarctica, no better route could have been selected. There can be little doubt that the expedition would have been completely successful and reached the Pole but for the accidents to the ponies. Four of them died in the winter quarters, one from eating some poisoned shavings, and three from eating sand—perhaps due to the craving of horses for salt, that may not have been adequately allowed for in their food. The most irreparable accident was the loss of the last pony during the southern sledge journey by its fall into a crevasse on the Beardmore Glacier. The sledge party was thus deprived of an important part of its reserve food, and the accident was especially annoying, as the pony was to have been killed that night. The horse meat was not a complete success, as it brought on dysentery. Sir Ernest Shackleton explains this somewhat unexpected result by the meat being poisoned by a toxin of exhaustion. As the symptoms of fatigue can be transmitted by inoculation from a tired to an untired dog, the suggestion sounds probable.

The work is prefaced by an admirable introduction by Dr. Mill on previous Antarctic work. The first volume describes the equipment, the vain attempt to land on the eastern side of the Great Ice Barrier, the establishment of headquarters on MacMurdo Sound, and the winter's work there. The motor car proved of great service around the station, but though it ran well on smooth sea ice, it would have been of no use on the soft surface of the Barrier.

The great sledge journey to the south was, therefore, dependent upon the ponies; and these did their work well. The sledging party consisted of Sir Ernest Shackleton, Adams, Wild, and Marshall, with

[1] "The Heart of the Antarctic. Being the Story of the British Antarctic Expedition, 1907–9." By Sir E. H. Shackleton, C.V.O. With an Introduction by Dr. Hugh Robert Mill. Vol. i., pp. xlviii+372; 132 plates. Vol. ii., pp. xvi+419; 141 plates, 3 maps. (London: W. Heinemann, 1909.) Price, 2 vols., 36*s.* net.